Reforming the Speed of Justice: Evidence from an Event Study in Senegal

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This paper illustrates the role of justice reforms in increasing the celerity of adjudication. Theoretically describing judges' incentives provides a taxonomy of the nature of legal delays (*idle* or *strategic*) central to designing effective legal reforms. We examine these theoretical predictions by conducting an event study of a reform that sets a deadline on the length of civil and commercial pre-trial procedures in Senegal. We exploit the staggered rollout across the seven civil and commercial chambers of the regional court of Dakar and high-frequency data on the treatment of the 2012/15 caseload. We find a large reduction in the length of the pre-trial stage of 73 days (0.5 SD). The effect is similar for small and large cases, and is attributable to an increase in the *decisiveness* of each hearing, as the number of fast-tracked cases increases, the number of case-level pre-trial hearings is reduced, while judges are more likely to set hard deadlines. These results are in line with a model in which judges *idly* delay cases. These gains in speed do not come at the cost of quality.

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I. Introduction

The speed of justice is typically referred to as a key indicator of a country's business climate and figures at the core of the *Doing Business* indicators (The World Bank Group, 2011).[2] Whether to start or close a business, register property (including intellectual), protect investors or enforce contracts, firms need to rely on the legal system. Stronger institutions lead to higher levels of investments (Pande and Udry, 2006; Le, 2004; Rodrik, 2000 and 2005), and capital accumulation drives a higher growth rate (Barro, 1991; Mankiw, Romer, &Weil, 1992; Solow, 1956). Consequently, slow justice delivery is associated with a poorer business climate.

Economic governance policies in developing countries often aim to increase the speed of commercial justice. Yet, court-level interventions susceptible of cutting delays are rarely rigorously evaluated (Chemin, 2009b). Most legal reforms are rolled out non-randomly across courts, judges or cases. Coupled with aggregated, annual data, the evidence linking faster justice to investment often fails to establish causality (Aboal et al., 2014).

We present case-level evidence on the causal impact of a legal reform designed to increase efficiency and reduce delays in court. Using rich, high-frequency administrative data on the 2010/15 caseload in the court of Dakar, Senegal, we show that simply imposing procedural delays has large, positive, and significant effects on the speed of civil and commercial justice. We exploit the richness of the caseload data to isolate the mechanisms underlying these effects, such as the channels through which judges intensified the procedure and quality vs. quantity tradeoffs. A simple theoretical framework then allows us to

[2]For a more exhaustive review of the indicators of quality for the justice system, see Dankov et al.(2003).

characterize pre-reform court delays as mostly "idle", resulting from judges' failure to enforce a timeline, rather than "active", or motivated by career concerns of personal gains.

Senegal offers a good context to study the effect of a reform in court procedures, for three reasons. First, Senegal is a civil law country, which implies a relatively a high degree of formalism and, therefore, lengthy procedures (Djankov et al., 2003). Senegal ranked 142 out of 189 economies in the "contract enforcement" category of the 2014 Doing Business Report, suggesting a significant margin of improvement in the speed of commercial dispute resolution.

Second, the Ministry of Justice introduced a decree, in 2013, aiming to accelerate the speed of the civil and commercial dispute process. The decree changed the civil and commercial procedural code so as to empower judges to reduce the formalism and enforce submission of supporting evidence from the outset, apply pressure on the parties along the process, and enforce a four-month limit on the duration of pre-trial hearings—which historically accounted for over two thirds of the total duration of a case in first instance. While the decree was passed in July/August 2013 by ministerial vote and published in the *Journal Officiel* in October 2013, its application was staggered across the various chambers of the regional court between November 2013 and April 2014. Combined with our high-frequency case-level data, this gradual administrative rollout allows us to exploit an *event study design* to identify the causal impact of the reform on the speed of justice.

Third, we exploit four years of high-frequency data on the civil and commercial caseload in the regional court of Dakar to identify the mechanisms through which the reform affected the celerity of adjudication. We use a simple economics of career concerns framework (Dewatripont, Jewitt and Tirole, 1999a and 1999b) to formulate testable hypotheses on

judges' incentives as career bureaucrats, and take them to the data. We then take these predictions to the data, taking advantage of our rich court records to shed light on the mechanisms linking a change in the legal text to an increase in the speed of dispute resolution.

This study contributes to four strands of the literature. First, we add to the literature on public service reform by formally documenting the impact of a national change in civil and commercial procedure. Celerity of court adjudication is a well-tested measure of public service efficiency (Lichand and Soares 2014; Ponticelli 2013; Visaria 2009). We exploit a legal reform that imposed deadlines on the duration of the purely administrative part of the trial (pre-trial hearings) to study judges' incentives and the drivers of procedural waste. We follow Bandiera, Pratt and Valenti (2009) and establish an *idle* vs. *strategic* delays framework. In our setting, judges derive no benefit from *idle delays*, while *strategic delays* may create some private value. Motivated by a simple model of career concerns in the presence of multi-tasking (Dewatripont Jewitt and Tirole 1999), we exploit changes in the distribution of delays pre- and post-reform to identify the nature of procedural waste.

Second, we innovate on the existing judicial reform literature. Chemin (2009a) uses yearly court-level data to identify the impact of a legal reform in Pakistan, exploiting district-level variations in coverage. We use within-court variation in coverage and high-frequency case and hearing-level data to construct an event study around a change in legal procedure. This allows us to isolate the causal impact of the reform on the speed of civil and commercial justice.

Third, we bring new evidence on the determinants of judicial efficiency. Court-level studies tend to be circumscribed to richer economies (Chang and Schoar, 2006), and have limited case-level data (Coviello et al., 2015). In fact, the most granular data is typically judge-month statistics (Chemin, 2009a&b; Lichand and Soares, 2014; Ponticelli, 2013). In contrast, we have full access to audience and case-level data from the Regional Court of Dakar. We use these data to build a high-frequency panel of all cases that entered the court between 2010/2015, and retrace a full record of all procedures and hearings they underwent from entry to final judgment. This allows us not only to document the impact of the reform on the overall speed of justice, but also provide evidence on the underlying mechanisms and differentiate *intensification* from increased *decisiveness* of the procedure.

Last, we build evidence on the behavioral effects of deadlines. Delays in court may result from strategic behavior on the judges' part, whereby additional procedural time yields more precise evidence or higher likelihood to extract rents. Alternatively, they may just be a manifestation of *irrational* procrastination (Akerlof 1991). The reform we study is akin to the deadline experiment proposed by Chetty et al. (2014) in which they manipulate the delay for journal referees to complete their review. An important difference is that, in our set up judges are not explicitly reminded of the deadline at any point—hence not "nudged" into action close to the deadline through the use of reminders. Our results can be interpreted purely as a response to a change in the default delay within which judges should complete their pre-trial hearings. Finally, we test for "tunnel vision" (Mullainathan and Shafir 2013), whereby setting tight deadlines on one activity may increase the quality of the output subject to the deadline, while reducing performance on other tasks.

We find the reform positively affects the speed of justice by both reducing the formalism of the civil law system, and increasing the efficiency of overall procedure. In line with

Bandiera, Pratt and Valenti (2009), judges' reaction to the new reform corroborate the idea that *idle delays* were the main source of pre-trial inefficiency. We find a large reduction in the length of the pre-trial stage of 73 days (0.50 SD). We show that this effect is attributable to an increase in the *decisiveness* of each hearing, as the number of fast-tracked cases increases, case-level pre-trial hearings are reduced (0.21 SD), while judges are 68% more likely to set hard deadlines. We investigate possible speed-quality tradeoffs, and find no evidence of judges' effort displacement from deliberations to pre-trial stages: decision hearings are scheduled at the same speed, the overall number of hearings does not increase, and quality of the evidence does not seem to be affected by the pre-trial reform.

The remainder of the paper is organized as follows. We provide some element of background on Senegal's justice system and the legal civil and commercial procedure in Section 2. Section 3 places the reform in the context of the Senegalese civil and commercial code of procedure. Section 4 details the data and the event study design central to our identification. Section 5 presents the main empirical results, and Section 6 concludes.

II. Civil and commercial law

1. The court

Our study takes place in the regional court of first instance of Dakar, Senegal. Senegal is a civil law country, and judges are organized in chambers, consisting of a president and two additional judges, which together form a collegiality. While the court of Dakar covers the full range of legal practices, we focus on civil and commercial justice. At the beginning of our study period, in 2012, there were 4 commercial and 3 civil chambers in the tribunal of Dakar (Table 1).

Commercial and civil procedures in the tribunal of first instance consist of the following general steps (see Annex A for a full schedule of the procedure): referral (*saisine*), enrollment (*enrôlement*), distribution (*répartition*), pre-trial hearings (*mise en état*), decision (*délibération*), and judgment (*jugement*). Referral and enrollment are purely administrative steps that do not require the involvement of the judges. Distribution consists in the assignment of the new caseload to the chambers by the president of the court; it is notionally made on the basis of existing caseload and, to a limited extent, the specialization of each chamber. Chambers follow a schedule of hearings. Each chambers disposes of two dates per month on which hearings can be scheduled. Each hearing opens with the assignment of new cases to pre-trial judges, chaired by the president of the chamber. Next, each pre-trial judge chairs her scheduled pre-trial hearings. Finally, the president of a chamber chairs decision hearing, attended by the parties and all judges serving in that chamber (*collegiale)*. On average, a chamber takes in 16new cases at each hearing (bi-monthly), ranging from 9.1 to 26.8 across chambers and years (Table 1).

Over the 2012/15 study period, two chambers closed: the 4th commercial in 2015, and the 2nd civil in 2014. These closures led to increases in the size of the ongoing portfolio in other chambers, as their ongoing cases were redistributed across the tribunal by the court president. These changes in portfolio are uneven across chambers, due to a certain degree of specialization of each chamber (Table 1).

Commercial and civil disputes vary widely in their nature and complexity. Commercial cases include payment and contract dispute, as well as expropriation and collective actions against a moral person (firm). Civil cases include contract and payment disputes between individuals (e.g. landlord and tenant), as well as other civil issues like divorces. The average size of commercial and civil disputes in our sample is of CFA 75,604,000 (or about

157,000 US dollars), ranging from CFA 75,000 to CFA 8,700,838,000 (about 160 US dollars to 18,098,000 US dollars; Table 2).

2. Procedure

In this section, we now provide a simplified overview of the first instance civil and commercial procedure in Senegal, focusing on the pre-trial hearing and decision stages leading up to the publication of a judgment.

Pre-trial phase. At its first hearing, a case is assigned to a pre-trial judge by the president of the chamber, starting the pre-trial phase. During this phase, the parties are invited to build up their case. Pre-trial hearings are chaired by the assigned pre-trial judge who hears the parties and facilitates communication across sides. Consequently, the parties present their arguments, supporting documents, and procedural pleas, and additional expert reports may be ordered.[3] On average, a chamber has 146 ongoing pre-trial cases per hearing, ranging from 37 to 225 across chambers and years (Table 1). Throughout this phase, the judge is required to show impartiality while the parties are expected to build their case and, therefore, does not directly influence the quality of the parties' arguments. The outcome of a pre-trial hearing is either the referral of the case to an additional pre-trial hearing, or the conclusion of the pre-trial stage.[4] The judge can mark a referral as "strict" or "final" to communicate to the parties the urgency to conclude the pre-trial hearings. The pre-trial phase ends when the judge declares it closed and sends the case to the decision

[3] In practice, the documentation is presented in written form by handing one copy to the opposing party and the other to the pre-trial judge for inclusion in the case file.
[4] Other, rare, hearing outcomes (both in the pre-trial and the decision stage) are a nullification of the case at the request of the plaintiff and an amicable adjudication at the request of the parties.

stage by scheduling a decision hearing.[5] Before the reform, a case underwent on average 8.08 pre-trial hearings over a 153.03 day period (Table 2).

Decision phase. In the period preceding the decision hearing, the three judges of the chamber individually review the case file and meet in closed sessions to discuss the arguments put forward by the two parties. There are three possible outcomes to the closed-session deliberations of the judges. If a conclusion is reached, the judgment is pronounced. If the judges need more time to come to a conclusion, the decision is postponed and the date of the next hearing is announced. If the review of the case file reveals that the pre-trial failed to collect the evidence necessary to come to a conclusion, the case is referred back to the pre-trial stage ("pre-trial insufficient"). These outcomes are announced in the presence of the parties during the decision hearing. On average, a chamber has 54 ongoing decision cases per hearing, ranging from 3 to 107.3 across chambers and years (Table 1). Before the reform, an average case was deliberated and judged in 2.35 hearings over a 57.15 days period (Table 2).

Shortly after the judgment is pronounced it is made available to the parties, ending the first-instance proceedings.

III. Reform of the pre-trial phase

The legal reform at the center of our study stipulates explicitly its goal of speeding up dispute resolutions to attract investors and private equity funds (Ministère de la Justice, 2013). The decree (n°2013-1071, dated August 6, 2013) was adopted by ministerial council

[5] During the pre-trial phase, the case file is kept at the enrollment office and is only seen by the pre-trial judge briefly during the hearings when adding to it written arguments and evidence brought forth by the parties. The first time the pre-trial judge assesses the case file in its entirety is when he/she verifies its completeness at the end of the pre-trial (*vérification*). In contrast, the case file stays with the judge throughout the decision stage.

on July 18, 2013. It modified the civil procedural code to address both supply and demand-side bottlenecks in the pre-trial procedure, in three main ways: first, it enforced a four-month limit on the duration of the pre-trial procedure; second, it assigned new powers to pre-trial judges; third, it required the parties to take active part in the procedure.

First, it imposed a four-month limit on the length of the procedure. This maximum delay was put in application for all ongoing cases in a given chamber at time of application, although the text recognized that it could not be retroactively applied where cases were close to, or over, the four-month deadline. Before the application of the decree in 2012/13, only half of all cases completed the pre-trial procedure in four months or less (Table 2).

Second, judges have more leverage to speed up pre-trial hearings. Specifically, it allows judges to exert pressures on the parties to avoid dilatory actions, by imposing stricter delays on pre-trial hearings, managing more closely additional expert reports and inquiries he may have requested from the parties, and allows judges to declare a case *inacceptable* in the very beginning of the pre-trial.[6] Second, additional "circuits" are created, allowing urgent cases to be judged at the outset, without undergoing pre-trial hearings. Again, the decree required that these measures be applied to the ongoing caseload at time of application, recognizing that it would not be applicable to cases "further along" the procedure.

Finally, defendant and plaintiff sides are asked to cooperate and be active participants throughout the pre-trial procedure. This enhances the power of the judge by increasing the degree of transparency of the procedure. First, both parties are empowered to demand that

[6] In the previous version of the code, pre-trial judges could not dismiss a case brought forward without sufficient supporting evidence. Instead, such cases would undergo the pre-trial procedure, during which the supporting evidence would fail to be assembled to build sufficient evidence to proceed to the deliberations.

the opposing side present supporting documents on an ongoing basis over the course of the procedure. The judge would set a reasonable deadline for presentation of the evidence, and a case could be nullified should the party fail to provide the evidence within this delay. Second, the reform grants each party direct access to the opposing party's witnesses. This is a break from the previous text, whereby judges were sole responsible for witness interrogation.

IV. Court data

We have full access to administrative data on civil and commercial caseload across all chamber of the first-instance court of Dakar, Senegal, over the 2012/15 period. Digitizing these records is at the core of our contribution, as court data were only available in paper form at the onset of the project. In the context of the World Bank's Economic Governance Project, we worked with a team of court-based enumerators to digitize all archives going back to 2010 and set up a real-time data entry for the ongoing caseload. This thorough data capture effort allows us to observe steps in the legal chain along two dimensions.

First, we observe all hearings held by civil and commercial judges, with a full record of which cases were heard in each hearing, at which stage of the procedure, and the corresponding decision taken during the hearing. These include pre-trial and decision hearings (Section 2.2.). Hearings are scheduled on a bi-monthly basis, on a chamber-specific schedule that is set every 6 months by the president of the court; this yields 21 hearings per chamber per year, after removing the summer break.[7] All pre-trial judges in a given chamber must hold hearings at the dates set in the schedule. Yet, not all ongoing

[7] A six-week summer break is established at the chamber level over the three-month period August-October, on a rotating basis across chambers, and all judges in a given chamber must take leave during this period.

cases must be heard at every hearing, yielding variations in both length and intensity of the procedure across cases.

Second, we have access to the full caseload for the January 2012-June 2015 period. For each case, we have a full record of when it entered in the court, when it was transferred to a chamber to start the pre-trial procedure (first hearing), when it finished the pre-trial procedure (last pre-trial hearing), what was the outcome of each pre-trial and decision stage hearing, which type of final decision was taken and when, and the judge in charge of the case at every hearing, as well as the contested amount.

We use these two sources of data to retrace case-level and audience-level history for the entire caseload that entered the court over the 2012/15 period. This yields an analysis sample of 5,169 cases. We run our analysis at two levels. First, we construct case-level outcomes, collapsing hearing data at the level of the case. This allows us to gauge both the celerity and the quality of the procedure, at pre-trial and decision stages separately. Outcomes describing the speed and intensity of the procedure include duration of the procedure, number of hearings, probability to complete the stage within the legally set delay, and the probability for a case to be heard at any audience over the course of the procedure. The probability that the evidence amassed during the pre-trial stage is declared insufficient at the decision stage offers a measure of pre-trial quality; the probability that a decision is postponed to allow judges more time to review the evidence, a measure of judges' effort at the decision stage.

Second, we build hearing-level outcomes, collapsing all chamber-hearing-case level outcomes at the chamber-hearing level. This yields a sample of 21 hearings per chamber

per year. We use these data to describe the inflow of cases in each chamber and in the court over time.

V. Theoretical framework

Understanding the main drivers of the speed of justice requires understanding the incentives faced by the different actors of the legal procedure in the main *active* phases of a civil or commercial case: pre-trial and decision. We develop a simple theoretical framework of judges' incentives to formulate predictions on their response to the introduction to the decree. Second, we briefly discuss parties' incentives during the pre-trial phase.

1. Judges (*theoretical model under construction*)

Judges are career bureaucrats competing for promotion to the higher levels of the judicature. As such, their main incentive is to expend effort so as to convince their peers and superiors of their talent (Dewatripont et al 1999a&b). They operate in a two-task environment, choosing to allocate effort between managing pre-trial hearings, and deliberations. We now outline the incentives they face.

As outlined in Section 2, a judge's role on the in the pre-trial phase is limited to that of a broker who elicits the parties to prepare and share their arguments. At this stage in the trial, they cannot demonstrate their talent to others. Hence, pre-reform, judges only derive utility from *strategically delaying* the pre-trial stages by (1) increasing the precision of the evidence available for deliberations by increasing delays offered to the parties, and/or (2) extracting rents from the parties, the probability of which goes up with time spent in pre-trial. The probability of (1) increases with the multi-dimensionality (complexity) of a case, and (2), with the amount of the dispute.

Post reform, judges are given incentives to increase the speed of the pre-trial procedure. Hence, a simple model of judge's effort predicts that a *strategic* judge will reallocate effort from uni-dimensional (simpler) cases to multi-dimensional (more complex) cases. Specifically, strategic judges should differentially increase the duration for uni-dimensional (simpler) cases, decreasing the intensity of the hearing schedule for these cases, at no extra cost. Instead, a strategic judge will reduce duration of multi-dimensional cases, increasing the intensity of their hearings.

Idle delays occur when judges do not extract private benefits from longer pre-trial hearings, neither in (1) nor (2). In this setup, judges simply procrastinate and fail to set firm deadlines to the parties in pre-trial hearings. This procrastination comes at a cost to the judges, as it multiplies the number of hearings and, therefore, time they spend on each case. Hence, this is a case of irrational procrastination (Akerlof 1991). In this case, the reform simply nudges judges to adopt a new delay. They respond by decreasing duration of all cases and reducing the total number of hearings, and increasing the *decisiveness* of the pre-trial procedure. In this setting, the effect on the quality of the evidence and, therefore, of the decisions, is ambiguous.

2. Parties (*Defense and Plaintiff*)

In this paper, we take parties' behavior as given and focus on judges' response to a change in incentives to understand court mechanics. Hence we assume that parties engage in strategic bargaining given a deadline and, as in Ma and Manove (1993), have a non-zero probability of missing the deadline. This is in line with the nature of Civil Law systems, where judges have more power over the pre-trial process than in Common Law systems (Dewatripont et al 2000).

IV. Empirical strategy

We employ an event study design to capture the causal impact of a reform in the civil and commercial procedure code on the speed of justice in the regional first-instance court of Dakar.[8] We exploit the fact that, while the decree was ratified in July/August 2013 and published in October 2013, it was applied at different times across the 7 civil and commercial chambers of the regional court. The timing of the introduction across chambers is likely endogenous to chamber characteristics. We address this by using high-frequency data around these multiple cut-offs to identify the causal effect of the reform, net of all other contemporaneous factors, in a flexible difference-in-differences framework. Combining the staggered introduction of the reform across chambers with 3 years of pre-intervention data allows us to purge or estimates of seasonal effects, while controlling for chamber-level heterogeneity.

Our identifying assumption is that the introduction of the decree is the main source of variations in the speed of justice in the two years following the application of reform and that, in the absence of the reform, the speed of justice would have followed a steady trend both within and across chambers. There are two main threats to our identification: first, chambers may not follow parallel trends in the pre-decree periods; second, chamber and court-level structural changes may occur around the introduction of the decree. While our results corroborate the presence of a structural break in our outcomes of interest at the introduction cutoff, and while the event study design allows us to address these issues, we run the following checks.

[8] This approach is akin to that used by Jensen (2007), Guidolin and La Ferrara (2007), and Atkin et al. (2015).

First, we test the assumption that the profile of the incoming caseload is unaffected by the introduction of the decree. We run our structural break diagnostic across all five introduction cutoffs. For this, we regress the number of incoming cases at each chamber hearing on a post-entry dummy (treatment), a linear trend, and their interaction. The coefficients on the treatment variable are insignificant, whether or not we allow for an adjustment period (cols 1-2, Table 4). These results show no significant break in trend around these multiple cutoffs (see Figure 1 for a graphic representation). Second, we show that the number of cases that enter the court over time follows a smooth trend around these cutoffs (Figure 2).[9] Next, we verify that there is no change in composition of the caseload. For this, we show that the size of the claims cannot predict the introduction of the reform in a given chamber (cols 2-3, Table 1; Figure 3). Finally, we find no record of court-level changes in the structure of the chambers over our study period, other than the introduction of the decree.[10] These checks unanimously corroborate the validity of our event study design in capturing the causal impact of the reform on the speed of justice.

There is one potential source of bias that our design cannot address: chamber-level endogeneity of the application with respect to anticipated post-reform chamber-level structural breaks. [11] In this scenario, the different chambers decided on the timing of application of the decree in reaction to anticipated chamber-specific shocks. For instance, a predicted increase in the caseload specific to a given chamber may have led the president of that chamber to speed up application. Chamber-level structural changes are unlikely, since

[9] As noted in Section 2, the size of the incoming caseload varies across chambers. This is attributable to a certain degree of specialization in each chamber.
[10] The only change in the court is the closing of two chambers, as mentioned in Section 2. These closures do not coincide with any of our cutoffs. Since a reduction in the number of chambers implies a cut in the number of judges, these closures should dampen the effect of the decree on the speed of treatment.
[11] We should note that the size of the caseload varies by chamber. This is due to the degree of specialization of each chamber within the broad areas of civil and commercial justice.

the caseload is evenly distributed across chambers by the president of the court twice a month during the distribution hearing. Again, finding that the inflow of cases into each chamber remains constant across the different cutoffs, both on average and individually, indicates this was not the case (Figures 1 and 21).

V. Results

In this section, we examine the causal impact of the reform on the length and structure of the pre-trial procedure. We first present results on the overall effect on duration of the pre-trial procedure. Next, we use rich procedure data to document the channels through which the reform affected celerity, and evidence on quality vs. efficiency tradeoffs.

A. Duration

We estimate three main models to measure the impact of the decree on the speed and nature of court procedure. First, we document the effect of the decree by period of entry of a given case in court, around its chamber's cutoff. The intuition is that the decree was applied to the younger part of a judge's portfolio—the part that is still undergoing pre-trial hearings. Therefore, we should see a sharp increase in the speed of resolution for the cases having entered the court close to the application threshold, relative to those that entered earlier. In practice, we estimate a flexible functional form that assigns one treatment effect per case entry period, as follows

$$y_{ij} = \alpha + \sum_{\tau=-38}^{20} \beta_\tau \, 11(t\_application\_since\_entry_i == \tau) + D_m + D_j + \varepsilon_{ij} \quad (1)$$

y_{ij} is outcome of case i, in chamber j; $t\_application\_since\_entry_i$ indicates the number of hearing (half-month periods) between the entry of case i in court and the application of the decree in chamber j, where 0 is indexed to be the date of application of the decree in all

chambers (negative values indicates the a case entered before the application of the decree, while positive values refer entry after application); $11(condition)$ is an indicator function taking value one if $condition$ is met, zero otherwise; $11(t\_application\_since\_entry_i == \tau)$ is an indicator function that takes value one if case i entered τ periods away from chamber j's application of the decree.[12] D_m and D_j are calendar month and chamber dummies, and t is a linear trend. Standard errors are clustered at the ($chamber\ x\ period\ of\ entry$) level.[13]

Second, we use survival analysis to estimate the effect of the reform on case duration. In practice, we use a Cox proportional hazard model to estimate the hazard rate $h(t)$, of a case exiting pre-trial within four months of entry, conditional on the same covariates as in (1). This approach adds to the simple OLS estimation proposed in (1) in that it corrects for censoring bias without being subject to selection bias, conditional on baseline hazard rate $h_0(t)$. Here, failure corresponds to exiting the pre-trial stage. We estimate the following Cox proportional hazard model

$$h_{ij}(t|D_m, D_j) = h_0(t) \exp[\sum_{\tau=-38}^{20} \beta_\tau 11(t\_application\_since\_entry_i == \tau) + D_m + D_j] \quad (2)$$

$\hat{\beta}_\tau$ is now interpreted as the impact of entering the court at τ on the hazard of exiting pre-trial stage, relative to a reference dummy with a hazard ratio of one. Hence, coefficients below 1 imply a lower probability of exiting, and above 1, a higher probability.

[12] In the current version of the paper we restrict our analysis to a window of 38 pre-decree application and 20 post-decree application hearing periods. We use the January 2012-June 2015 data to construct the same time window around each of the chamber-level decree application dates, allowing for four months' time to complete the pre-trial stage. Hence, entry is restricted to February 2015. In a future version, we will extend the window to 1.5 years post-decree application (30 t).

[13] Our results are robust to a more stringent clustering at the chamber level.

Finally, we document the average effect of the decree across the cutoff, using one overall treatment dummy. For this, we estimate the following model

$$y_{ij} = \alpha \sum_{\tau=-42}^{21} 11(t\_application\_since\_entry_i > 0) + \eta t + D_m + D_j + \varepsilon_{ij} \quad (3)$$

We run the analysis on two samples: a full sample, and excluding an adjustment period of three hearings to purge our estimates of short-term adjustments.

Pre-trial phase

Did the reform affect the celerity of treatment in the pre-trial phase? We find evidence of a clear jump in pre-trial duration for cases that entered the chamber close to the application of the decree (Figure 4). The average effect indicates a reduction in the pre-trial duration by 34.77-46.01 days, depending on the inclusion of an adjustment period around the cutoff (cols 1 and 3, Table 4). This is a large effect, on the order of 0.24-0.32 of a pre-reform standard deviation. While the estimate of the average effect is biased downwards due to inevitable data censoring[14] (evidenced in Figure 4 by an overall downwards trend in pre-trial duration), controlling for a linear trend suggests that censoring cannot account for the observed jump in pre-trial duration. In addition, interacting a linear trend with our treatment variable corroborates the assumption of parallel trends around the decree introduction cutoff (cols 1 and 3, Table 4). Finally, we a Cox proportional hazard model to account for censoring in our measure of duration as expressed in (2). Our results indicate

[14] For any late entry cohort, the longest-lasting cases are still ongoing and hence omitted from this sample.

that the introduction of the decree significantly increased the hazard rate of a case finishing pre-trial by 18.7-30%.

The finding of a reduction in pre-trial duration is further supported by evidence of a similar jump in the likelihood of completing the pre-trial stage within four months (see Figure 2), an outcome that is not affected by censoring.[15] Recall that one of the decree's innovations was to introduce a fixed four month delay for the pre-trial hearings. On average, the likelihood of meeting this deadline significantly increases by about 15.2-21.6 percentage points, a 22-43.5% increase (cols 5 and 6, Table 4).

Our theoretical framework indicates that comparing the distribution of pre-trial durations across the application of the reform will shed light on the nature of the delays. We plot kernel distributions of procedural delays pre- and post-reform (Figure 6). The results are stark: after the decree is applied, the bulk of cases see their pre-trial shift to the left. This applies to all ranges of the pre-reform distribution. This is confirmed by a juxtaposition of densities across case cohorts (Figure 20). This hints that pre-trial delays were mostly *idle*, and that judges uniformly apply shorter timelines to all types of cases.

Decision phase

The decree explicitly targeted inefficiencies in the pre-trial stage of commercial and civil cases. We look into potential unintended adverse effects of the reform on court efficiency, whereby judges may have shifted effort from the decision stage to the now deadline-enforced pre-trial stage. Our results do not corroborate this notion. First, we do not estimate a significant jump in the duration of deliberations (Figure 7, and cols 1 and 2,

[15] Recall that the window of analysis (up to 8 post-decree application hearings) was chosen such that we observe four months of post-decree application data for all cases.

Table 5), the hazard rate of completing deliberations (cols 3 and 4, Table 5),[16] nor the likelihood of completing this stage within one month (Figure 8, and cols 5 and 6, Table 5). This confirms that the reform did not immediately affect deliberations. However, we observe that the introduction of the decree induced a significant change in the (linear) trend directing the speed of deliberation (cols 1 and 2, Table 5), towards a reduction in duration.

This positive effect of the pre-trial reform on the decision stage are all the more surprising that increasing the speed of the pre-trial led to an increase in the size of the decision caseload (Figure 19), as an increase in judges' workload is linked to an increase in delays (Coviello et al, 2014). These results indicate that the reform did not adversely affect the speed of deliberations. Instead, they suggest that an exogenously induced increase in judges' efficiency at pre-trial stages may have had positive spillovers on the decision phase.

B. Mechanisms

Our policy experiment does not allow us to causally unpack the mechanisms underlying the changes in the speed of justice. Instead, we use our rich case and hearing-level court data to shed light on the channels through which the decree affected duration in pre-trial and decision stages.

Pre-trial stage

First, we look at the number of pre-trial hearings cases undergo around the application of the decree. Figure 10 reports period-of-entry specific treatment effects, as estimated through (1). Similar to the effects on duration, we observe a significant and sudden decline

[16] While computing the hazard rate at pre-trial stage allowed us to fully account for right-hand censoring of the duration outcome, this is not true at decision stage. This is because our sample of decision cases is itself censored: it is restricted to cases that have made it out of the pre-trial phase but the time our data was last extracted (July 2015).

in the number of pre-trial hearings undergone by cases that entered the chamber close to the application of the decree. Cases entering a chamber after the decree experienced on average 1.42-1.91 fewer pre-trial hearings, equivalent to 0.22-0.30 SD (cols 1 and 2, Table 6). We also find a modest though significant jump in a case's likelihood to be heard at any hearing scheduled in its chamber over the pre-trial procedure, a 5.1-6.2 percentage point increase from a mean of 88.7% (Figure 11; cols 5 and 6, Table 6). Overall, these results suggest that the decree did not cut delays through intensification in the placement of hearings across a chamber's calendar, but rather by increasing the *decisiveness* of each hearing.

Second, we measure the impact of the reform on the extent to which judges started fast-tracking cases out of the pre-trial stages and into deliberations. Recall that the decree empowered judges to fast-track or dismiss a case for lack of evidence from the onset of the pre-trial procedure. We construct a case-level binary variable that takes value 1 if a case that entered a chamber altogether avoided the pre-trial procedure. We find that pre-trial judges made use of this new power, with a jump in the likelihood of cases experiencing an immediate decision (dismissal or judgment) increasing sharply for cases entering the chamber just before the cut-off (Figure 12). The average effect is large, a 18.3-23.8 pp. increase from a pre-decree mean of 13.1% (cols 3 and 4, Table 6). It is important to note that the sharp decline in duration presented in IV.A. is not attributable to an increase in fast-tracking, as results are robust to excluding fast-tracked cases from the sample.

Finally, we use hearing-level outcomes to retrace how many times a judge imposed a strict deadline on parties in non-decisive pre-trial hearings. Again, we find a sharp break away from the trend at the application of the decree (Figure 13). Since we now use a hearing-level outcome, the break appears at 0—the first hearing after the application of the decree in a

given chamber. This is a large effect, as judges are 5.5-6.2 pp. more likely to apply a strict deadline on one or both of the parties at the end of a non-decisive hearing, from a baseline of 15.4% (cols 7 and 8, Table 6). This result corroborates the idea that efficiency gains were made not through an intensification of the schedule, but an increased decisiveness at each step of the pre-trial procedure.

Decision stage

Recall that we find no evidence of displacement of judges' effort from decision to pre-trial hearings. Instead, we show a positive impact of the decree on the speed of the decision stage in the form of a shift in the linear trend governing duration. We now examine channels through which the decree may have reduced the duration of the decision stage. Cases that entered a chamber after the decree experience on average 0.37-0.54 fewer decision hearings (0.11-16 SD) than those that entered the chamber earlier (cols 1 and 2, Table 7). Again, the jump is not as clear as in the pre-trial phase (Figure 14).We see no effect on the probability of a case being heard at any scheduled hearing over the course of the decision procedure (Figure 15; cols 3 and 4, Table 7). This signals an increase in judge's effort at deliberation stage: cases are deliberated faster, in fewer hearings.

Quality of the pre-trial hearings

Finally, we examine potential quality-celerity tradeoffs in the pre-trial phase. As discussed above, the pre-trial procedure aims to prepare a case for judgment in the decision phase of the trial. We have access to two simple indicators of quality of a pre-trial process, marking different gradients of quality of pre-trial proceedings. First, quality can be expressed as the likelihood that a deliberation is broken, and the case is sent back to pre-trial ("pre-trial insufficient"). This relates to a very low quality of pre-trial proceedings. Instead, relatively

less prepared cases are likely to have their decision postponed at a hearing ("decision postponed"), instead of being marked with a judgment.

Figure 12 indicates no discernible jump in the probability that a case gets sent back to pre-trial after the introduction of the decree. This is corroborated by an insignificant and small average effect (cols 1 and 2, Table 8). Similarly, we find no significant change in the likelihood that a decision is postponed (Figure 13; cols 3 and 4, Table 8).

These results suggest that accelerating the pace of the pre-trial procedure did not displace judge's effort away from deliberations, and did not lead to a decline in the quality of the evidence. This corroborates the notion that pre-reform procedural waste was idle: judges did not extract any additional benefit, in the form of increased precision of the evidence, from running longer pre-trial procedures. In addition, we find that the reform does not lead effort to be reallocated to pre-trial hearings and away from decision stage: the number of decision hearings per case is reduced, and the decisiveness of each hearing remains constant across the decree application threshold—suggesting judges exert similar levels of effort in reviewing the evidence.

Finally, an important measure of quality of a decision (in first instance) is the probability that a decision gets appealed (Coviello et al, 2014). Our team is in the process of collecting these data and linking them to our first instance caseload, and we will present these results in a future version.

Heterogeneity

Central to our theoretical framework is the prediction that the reform may differentially affect multi-dimensional (complex) and uni-dimensional (simpler) cases. The intuition is

that, in a model of strategic delays, we should observe that the reform disproportionately reduce delays for simpler cases, with smaller effects on more complex cases. We use our rich caseload data to test this prediction, and allow the effect of the decree to vary with the size of the dispute (claim amount). We run a flexible form of (1), allowing for different treatment effects and trends across the median claim amount (Table 9).

First, our results confirm the idea that larger claim size is associated with longer procedure delays, on average. Second, we find that the decree equally increased the speed of both small and large-claim cases (col 1, Table 9).[17] More specifically, the impact of the decree on the likelihood of completing pre-trial in four months is identical across types of cases (col 2, Table 9). We also do not see a differential intensification of the hearing schedule for larger cases (col 4, Table 9). Turning to the channels, we find that judges are 10.3 percentage points more likely to apply pressure on larger cases after the decree, while the effect on smaller cases is not significant (difference significant at 10% level; col 5, Table 9). These results lend some support to the idea that judges applied the decree equally to all types of cases. Specifically, the absence of intensification of the procedure for large cases goes against theoretical predictions in the presence of a strategic manipulation of delays. Taken together, these results lend support to the notion that judges did not strategically manipulate delays for their own gains.

[17] Here, we run (1) with and without adjustment period, without notable changes in results, with the exception of our estimates on duration (col 1, Tables 8 and 9). Notice that our results on duration are quantitatively sensitive to the exclusion of the adjustment period, through a change in the post-trend for larger cases: the coefficient on the triple interaction changes sign across samples. This is attributable to the small post-reform sample size (Figure 22). However, this discrepancy does not affect our interpretation, as the effects remain significant for both types of cases, and do not differ in magnitude.

VI. Discussion

We formally document the impact of a legal reform on the speed and process of civil and commercial justice in Dakar, Senegal. The application of the decree was staggered over a 6-month period across the seven civil and commercial chambers of the court. We exploit this gradual rollout as well as rich, high-frequency hearing and case-level data over the 2010/15 period to construct an event study around each chamber's application date.

We find large effect on duration, and document that these efficiency gains were not made through intensification of hearings over shorter periods of time. Instead, cases that entered a chamber after the decree was applied experienced fewer hearings, with no change in frequency. Taking our simple theoretical framework to the data suggests that pre-reform delays were overwhelmingly *idle*, serving no private interest among judges. While, *de jure*, the decree affected the procedural code only at the pre-trial stage, we find that the efficiency gains spill over to the next (decision) stage in the trial. Again, we see no intensification of the hearings at decision stage, and rather a decline in the total number of hearings a case has to go through to reach a final decision.

The reform aimed to give judges more power to fast track cases out of the pre-trial phase, and to apply firm delay on the parties in order to meet a maximum 4 month pre-trial duration. We show that judges are more likely to use their newfound powers and fast-track cases out of pre-trial either for immediate decision or to dismiss them for lack of evidence.

Searching for additional cues in the data on the mechanisms through which delays were cut and deadlines adhered to, we find that judges were 46% more likely to apply strict deadlines on the parties in non-decisive hearings. Looking at markers of quality of the pre-trial proceedings, we find no effect. Overall, efficiency gains dominate, with positive

spillovers to the deliberation phase of the trial. More data will be added to explore these effects in appeal.

Taken together, our results suggest that, while judges in developing, civil law countries may face many constraints to productivity (Djankov et al, 2003; Chemin, 2009), simple changes in the procedure, such as a reduction in formalism and the application of deadlines, can be effective in increasing the speed of resolution. This suggests that, contrary to the model proposed by Coviello et al (2014), the *decisiveness* of legal proceedings offers a non-trivial margin at which legal reform can impact the speed of justice.

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Figure 1: Chamber-level incoming caseload

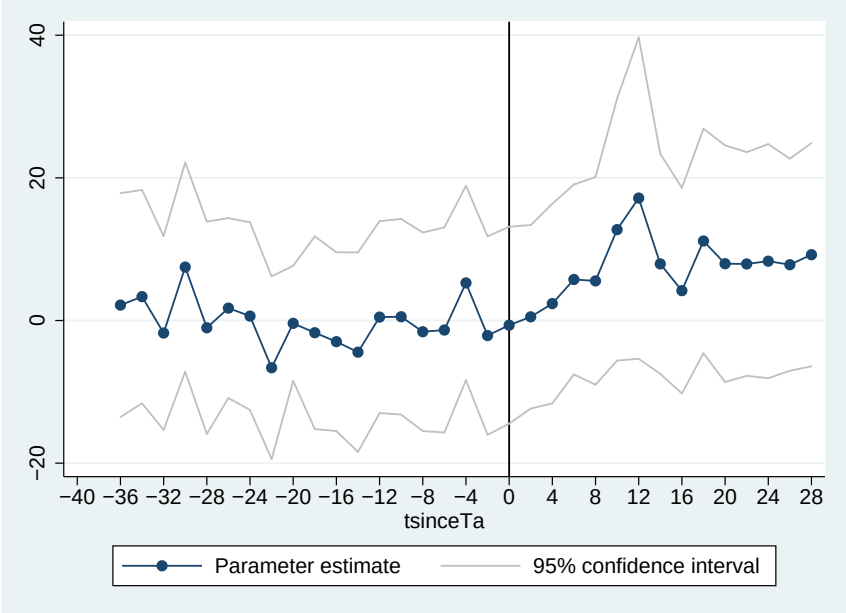

Figure 2: Type of incoming cases (above median claim amount)

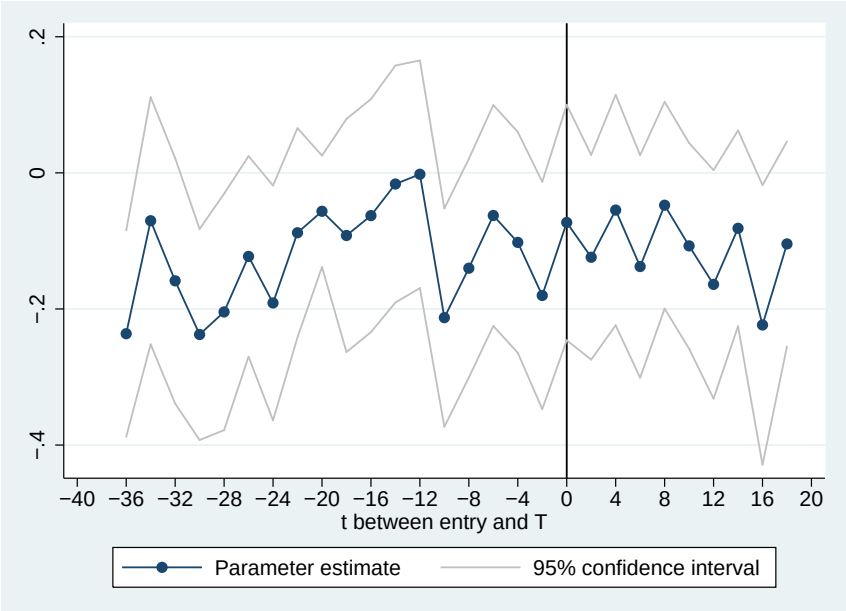

Figure 3: Courtwide incoming and ongoing caseload

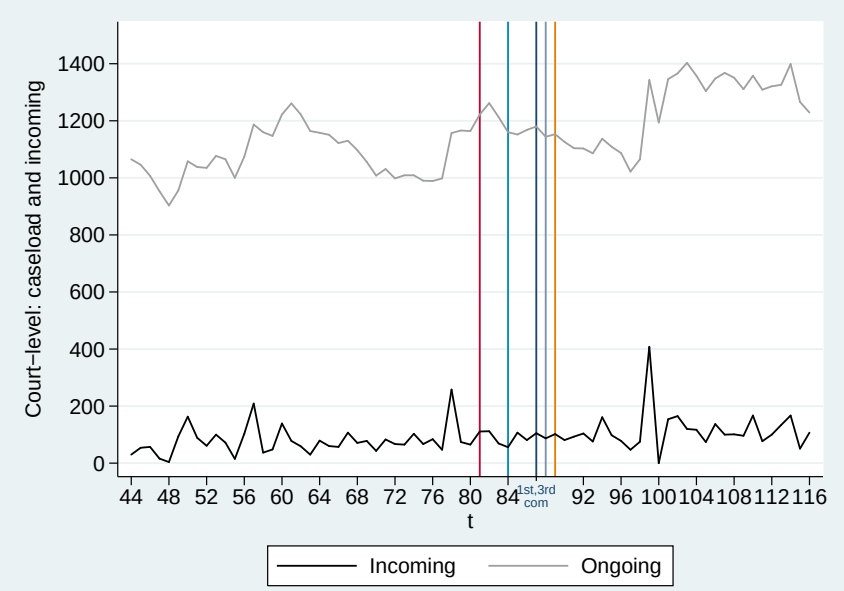

Figure 4: Impact on the pre-trial duration (number of days)

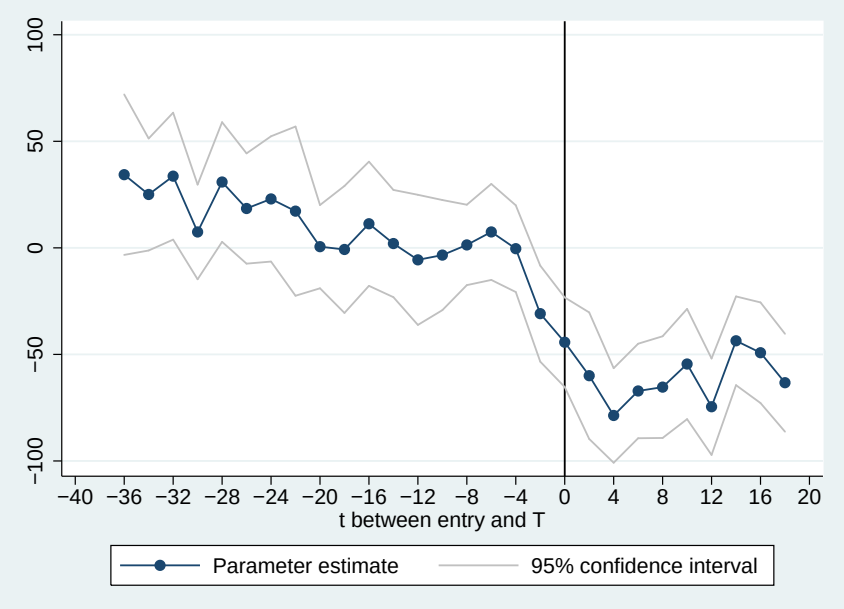

Figure 5: Impact on the likelihood to complete the pre-trial in 4 month

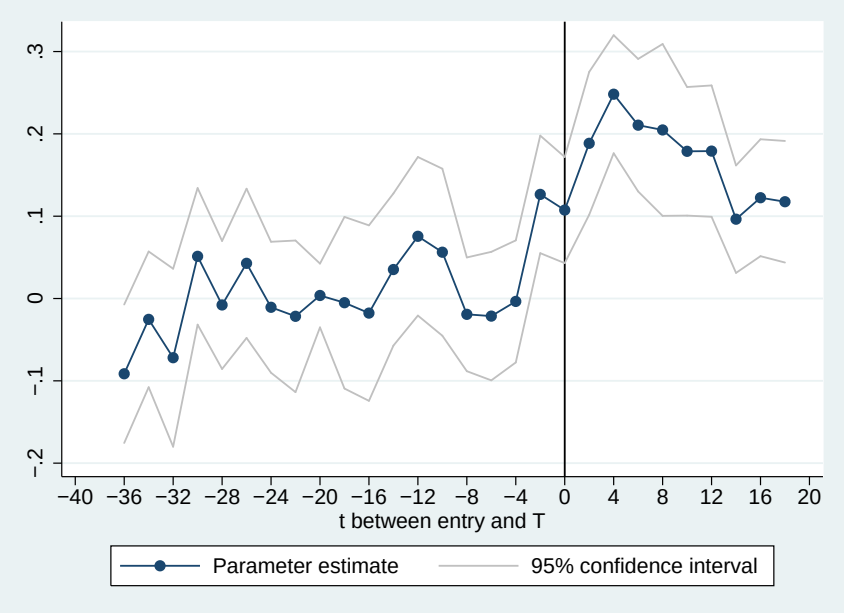

Figure 6: Pretrial duration: distribution

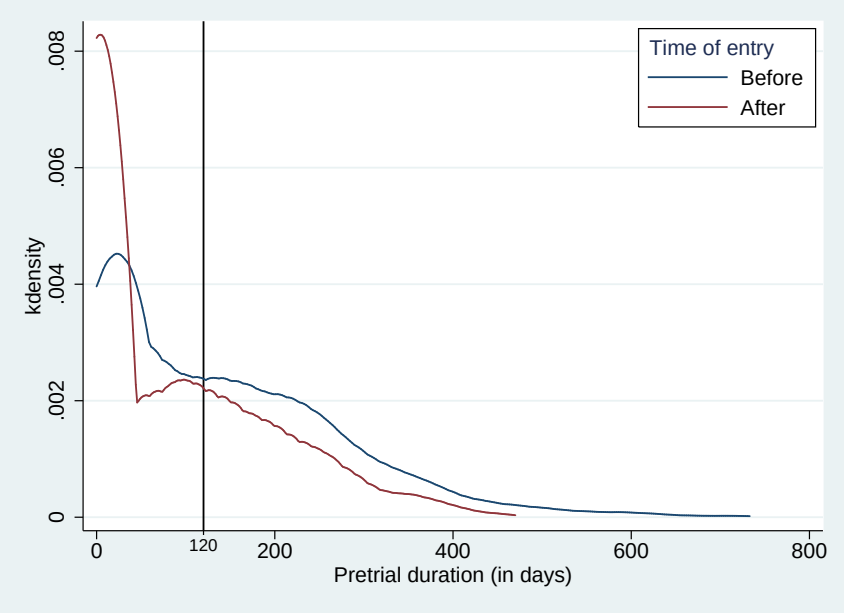

Figure 7: Impact on the duration of the decision stage (number of days)

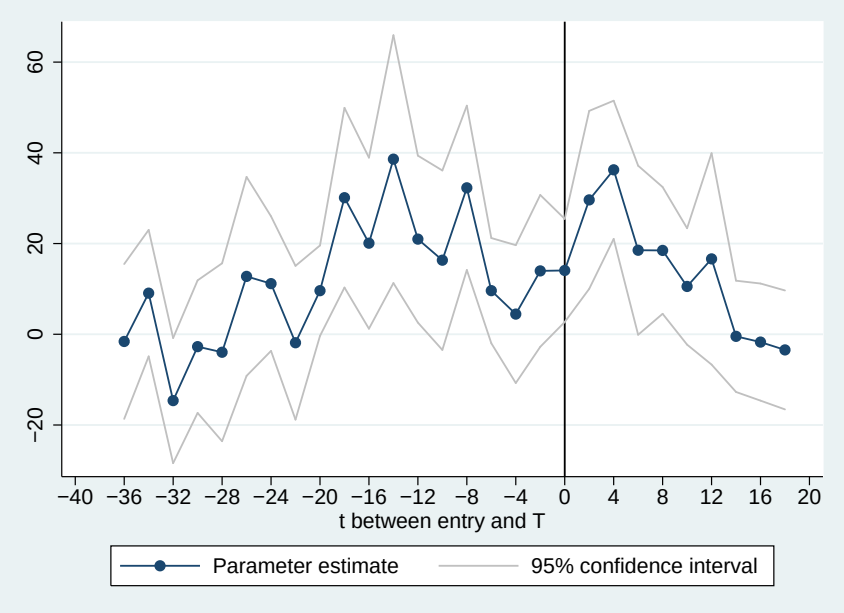

Figure 8: Impact on the likelihood to complete the decision stage in 1 month

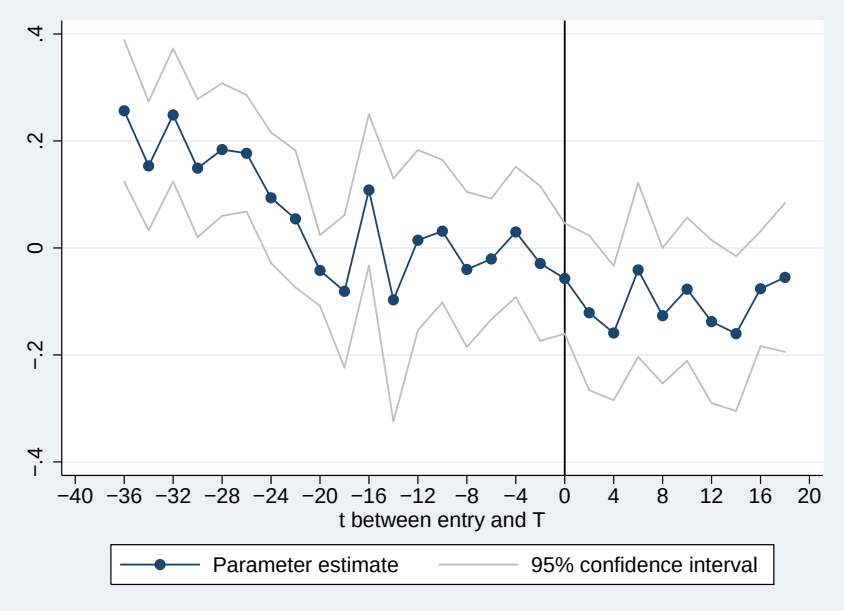

Figure 9: Impact on immediate decision likelihood (fast-tracked or inadmissible)

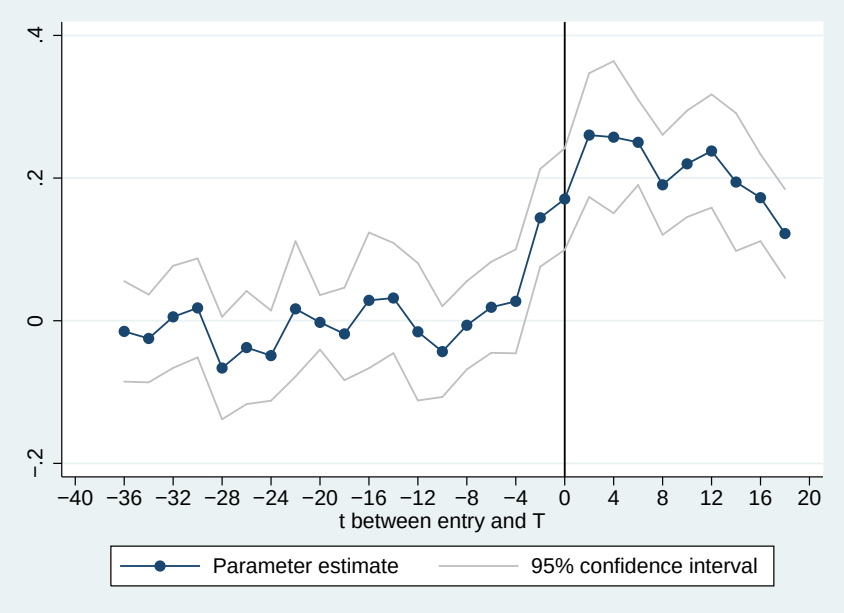

Figure 10: Impact on the number of pre-trial hearings

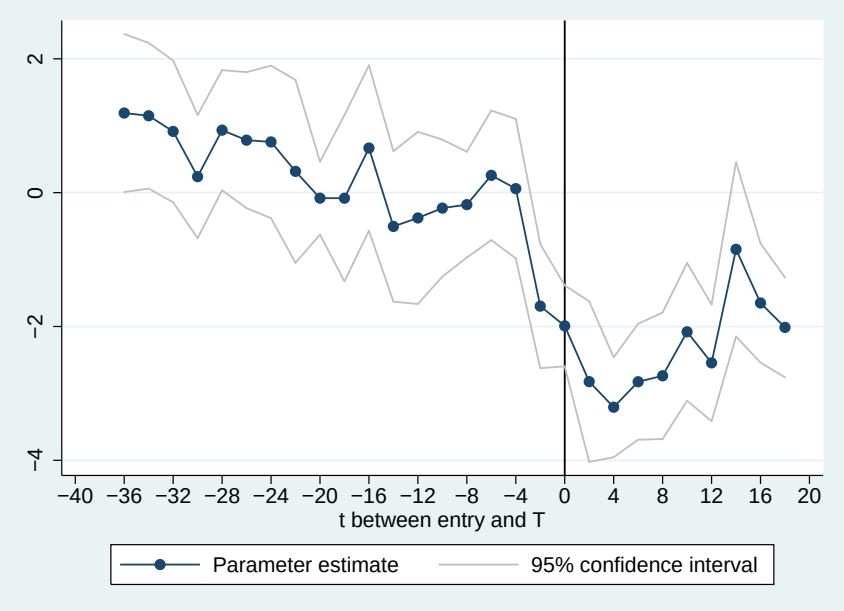

Figure 11: Impact on the pre-trial likelihood of being heard

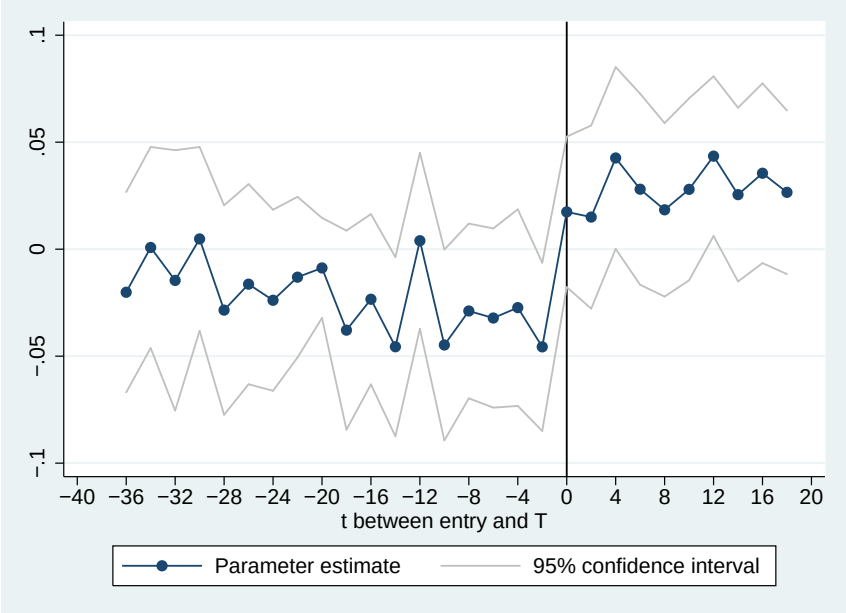

Figure 12: Impact on the judge being stricter in the pre-trial

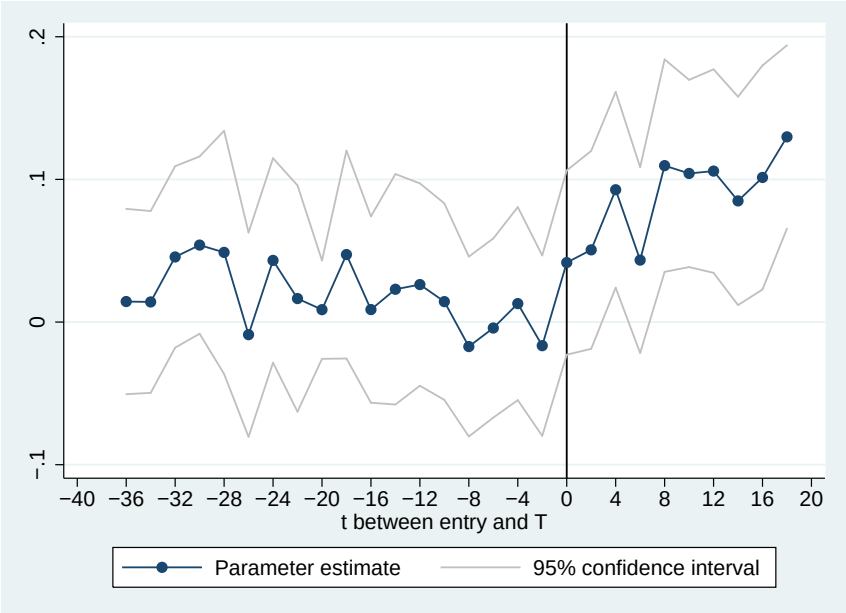

Figure 13: Impact on the number of decision stage hearings

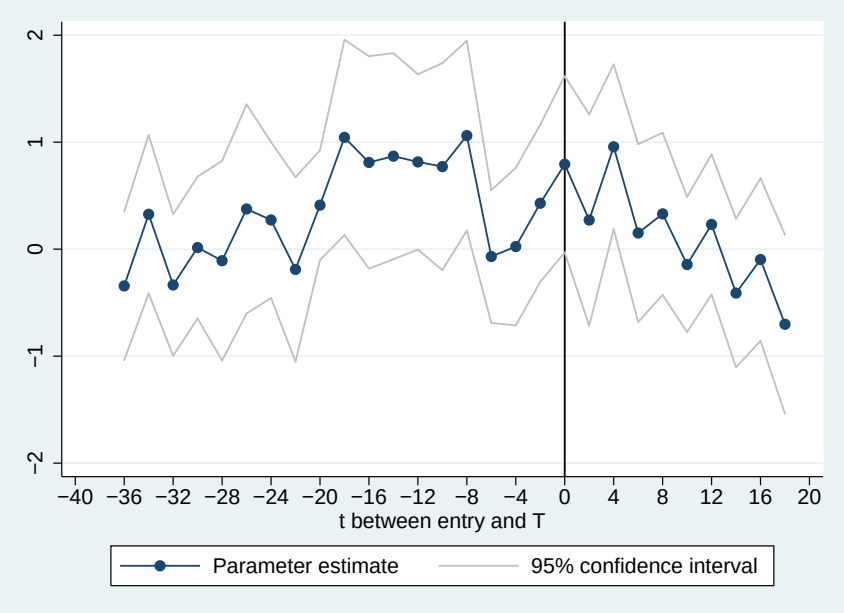

Figure 14: Impact on the decision stage likelihood of being heard

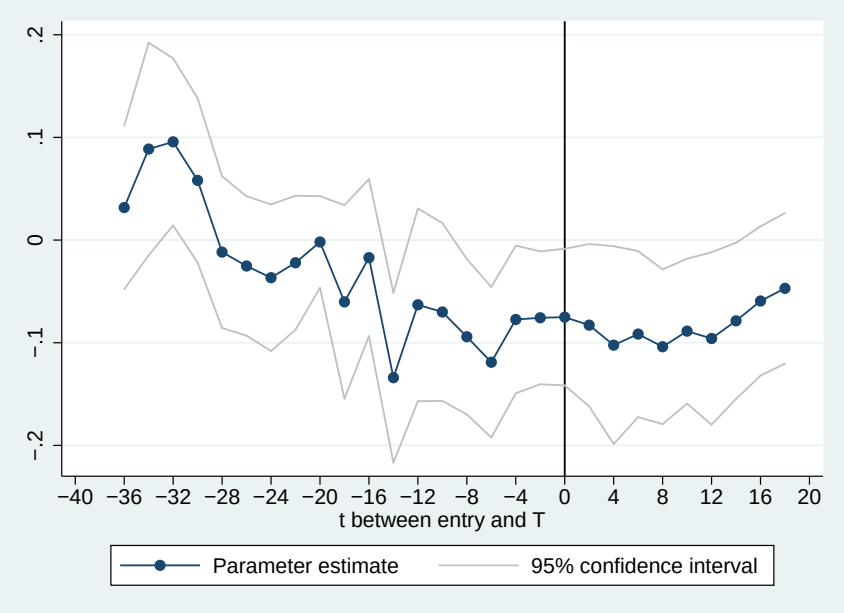

Figure 15: Impact on the likelihood of pre-trial failure

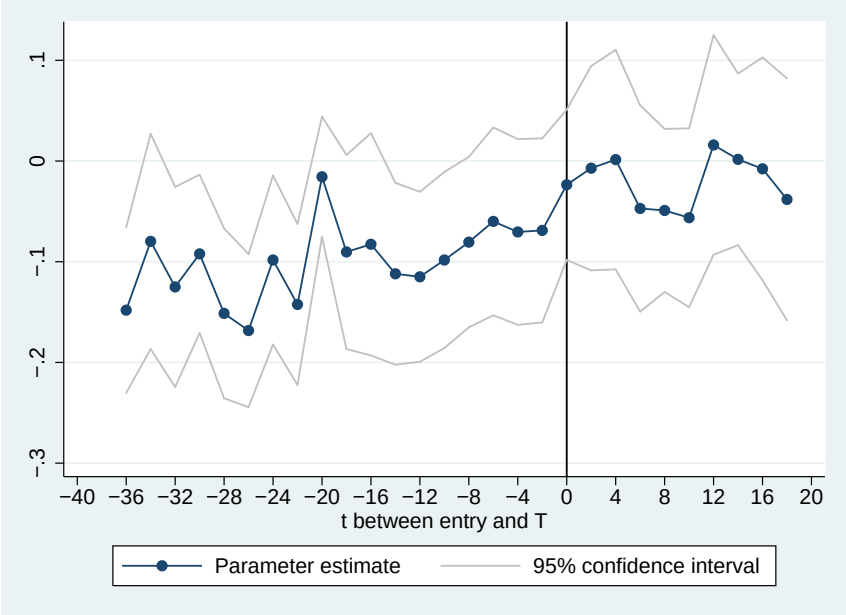

Figure 16: Impact on the likelihood of decision postponement

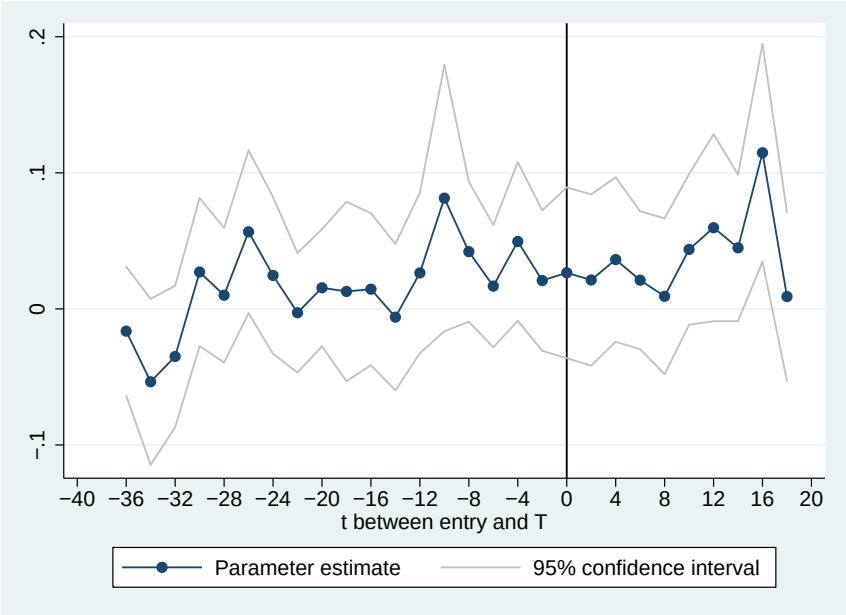

Figure 17: Hazard ratio: exiting pre-trial

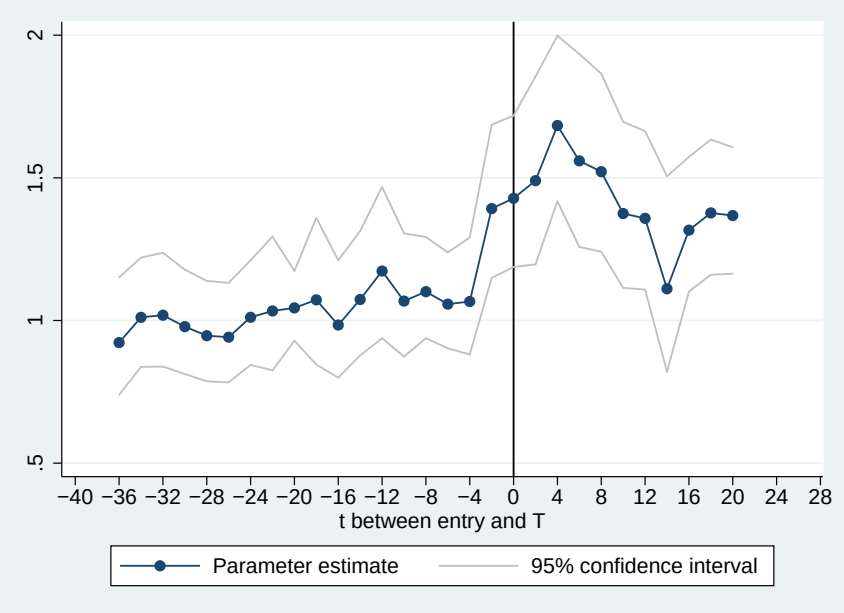

Figure 18: Kaplan-Meier survival estimates

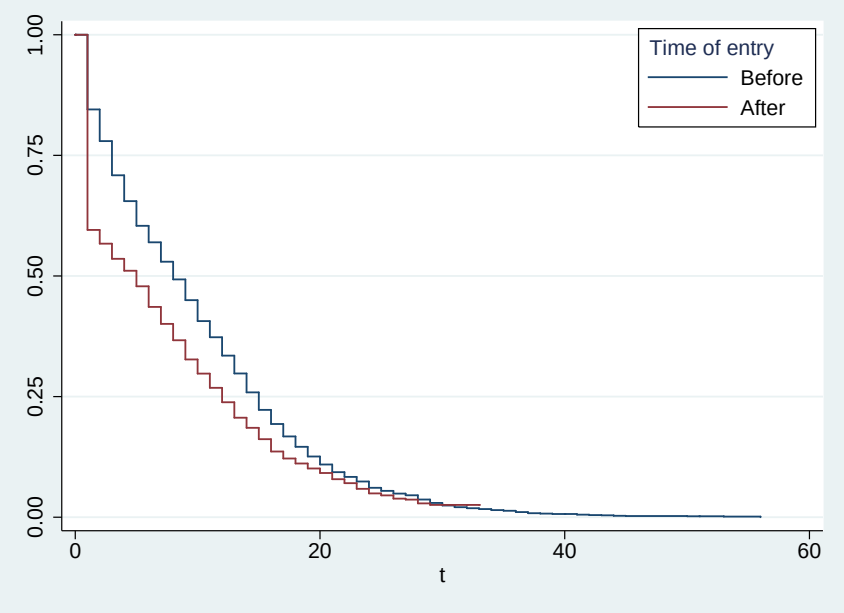

Figure 19: Ongoing cases in pre-trial and decision stages

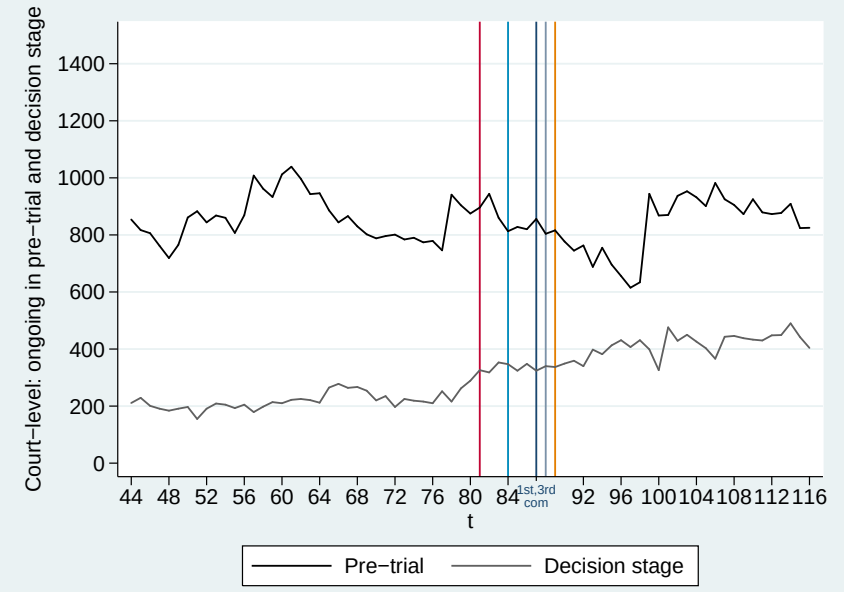

Figure 20: Pretrial duration: distribution by tranches

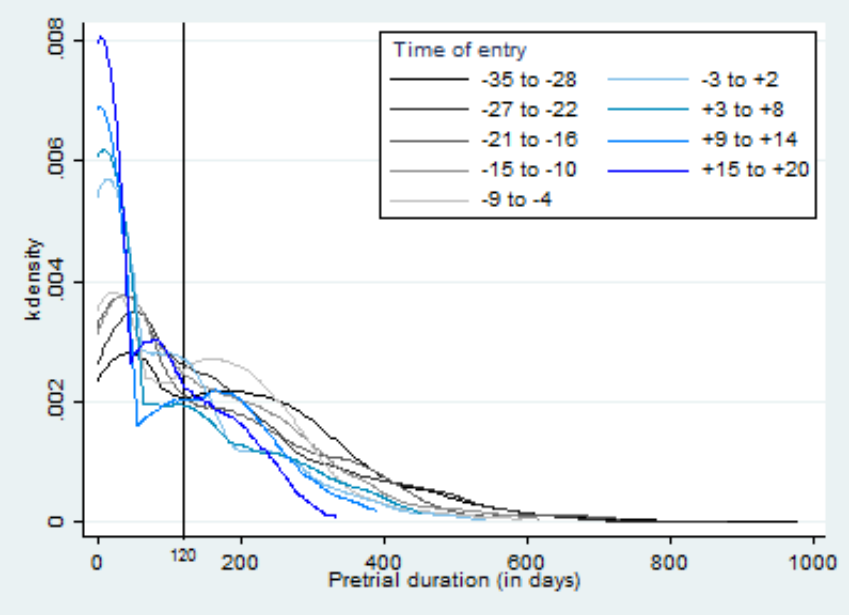

Figure 21: Chamber-level incoming caseload

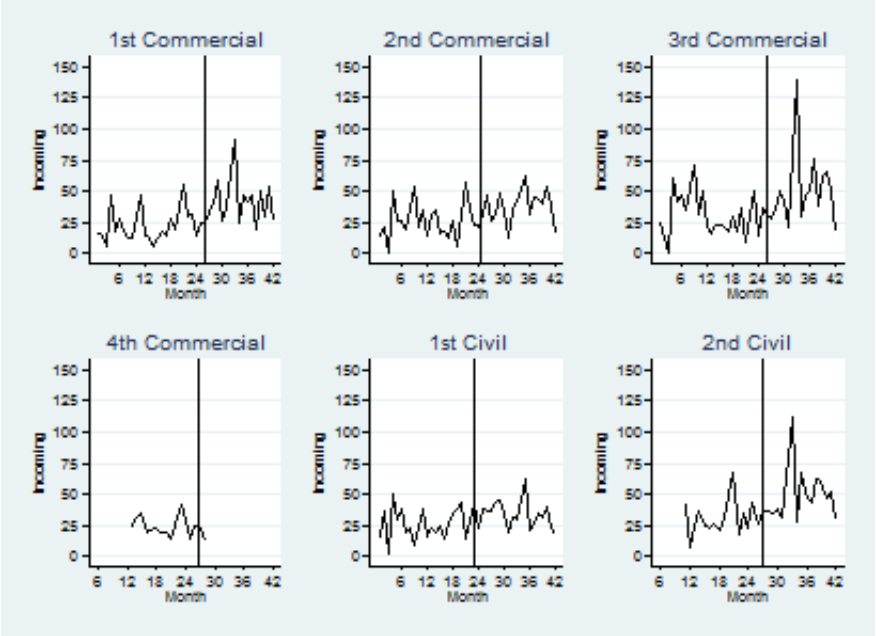

Figure 22: Differential effects - pre-trial duration

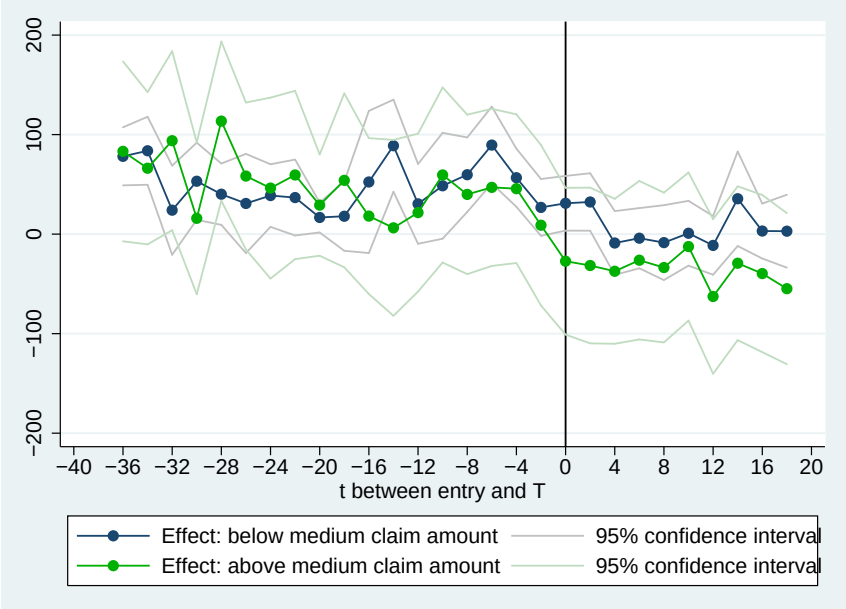

Table 1: **Summary statistics - chamber-level**

		1st Commercial	2nd Commercial	3rd Commercial	4th Commercial	1st Civil	2nd Civil	3rd Civil	Overall
Average number of incoming cases per hearing	2012	11.0	13.5	18.7	.	13.3	13.7	12.3	13.7
	2013	11.5	13.4	12.0	13.2	14.6	4.9	15.7	12.2
	2014	21.2	19.2	24.4	9.1	19.0	.	23.9	19.5
	2015	19.5	21.8	26.8	.	15.1	.	25.8	21.8
	Overall	15.8	17.0	20.5	11.2	15.5	9.3	19.4	16.4
Average number of ongoing cases in pre-trial	2012	140.6	188.1	149.1	.	225.1	165.0	37.0	150.8
	2013	116.0	209.1	109.3	63.2	196.2	86.4	89.7	124.3
	2014	153.4	201.2	142.0	70.3	156.3	.	123.1	141.0
	2015	174.1	262.1	155.5	.	144.8	.	134.3	174.2
	Overall	146.0	215.1	139.0	66.7	180.6	125.7	96.0	145.5
Average number of ongoing cases in decision	2012	24.7	26.1	46.9	.	50.5	48.6	3.0	33.3
	2013	26.8	48.7	44.6	16.8	68.6	32.7	31.1	38.5
	2014	48.7	97.1	84.0	27.0	99.4	.	45.9	67.0
	2015	60.5	107.3	101.4	.	85.6	.	64.9	84.0
	Overall	40.2	69.2	21.9	76.0	40.7	36.2	53.8	53.8

Table 2: **Summary statistics - pre-reform period**

	N	Mean	StD	Median	Min	Max
Duration of pre-trial hearings (in days)	2827	153.034	143.753	124.000	0.000	980.000
Likelihood of pre-trial completion in 4 months	2836	0.497	0.500	0.000	0.000	1.000
Duration of decision stage (in days)	2498	63.972	83.380	30.000	14.000	761.000
Likelihood of decision completion in 1 month	2529	0.494	0.500	0.000	0.000	1.000
Number of pretrial hearings	2836	8.081	6.402	7.000	0.000	41.000
Number of decision stage hearings	2538	2.622	3.470	1.000	0.000	36.000
No pre-trial	2836	0.121	0.326	0.000	0.000	1.000
Pre-trial likelihood of being heard	2814	0.887	0.131	0.923	0.286	1.000
Decision stage likelihood of being heard	2527	0.770	0.248	0.857	0.167	1.000
Judge more strict	2157	0.154	0.183	0.105	0.000	1.000
Pre-trial insufficient	2527	0.118	0.322	0.000	0.000	1.000
Decision postponed	2527	0.055	0.229	0.000	0.000	1.000
Claim amount (in 1,000,000 FCFA)	1773	75.604	390.238	8.000	0.075	8,700.838

Table 3: **Robustness checks**

	(1) Number of incoming cases (chamber level)	(2) Number of incoming cases (chamber level)	(3) Above median claim amount	(4) Above median claim amount
Hearing after decree	4.958	6.140		
application	(3.075)	(4.203)		
Trend	-0.088	-0.086		
	(0.104)	(0.113)		
Interaction	0.551**	0.460		
	(0.255)	(0.295)		
Entered after decree			-0.037	-0.038
application			(0.041)	(0.050)
Trend			0.003***	0.004***
			(0.001)	(0.001)
Interaction			-0.005*	-0.006*
			(0.003)	(0.003)
Constant	12.779**	6.572	0.357***	0.296***
	(5.695)	(4.992)	(0.053)	(0.061)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes
Pre-mean	14.190	14.190	0.501	0.501
Pre-sd	11.522	11.522	0.500	0.500
R-Squared	0.403	0.433	0.201	0.193
Observations	310	274	3532	3198

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by t (models (1) and (2)) or chamber-entry-t (models (3) and (4)). Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 4: **Impact on the speed of pre-trial procedures**

	(1) Duration of pre-trial hearings (in days)	(2) Duration of pre-trial hearings (in days)	(3) Hazard ratio - finishing pre-trial	(4) Hazard ratio - finishing pre-trial	(5) Likelihood of pre-trial completion in 4 months	(6) Likelihood of pre-trial completion in 4 months
Entered after decree	-34.772***	-46.011***	1.187**	1.300***	0.152***	0.216***
application	(10.315)	(12.927)	(0.090)	(0.128)	(0.034)	(0.045)
Trend	-1.490***	-1.212***	1.008***	1.006**	0.002**	0.001
	(0.294)	(0.344)	(0.002)	(0.002)	(0.001)	(0.001)
Interaction	0.956	0.909	0.986***	0.987**	-0.008***	-0.009***
	(0.672)	(0.803)	(0.005)	(0.006)	(0.002)	(0.003)
Constant	122.519***	167.044***			0.427***	0.407***
	(11.635)	(13.173)			(0.043)	(0.038)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes	No	Yes
Pre-mean	153.034	153.034			0.497	0.497
Pre-sd	143.753	143.753			0.500	0.500
R-Squared	0.191	0.201			0.127	0.130
Observations	4901	4405	5096	4594	5096	4594

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 5: **Impact on the speed of the decision stage**

	(1) Duration of decision stage (in days)	(2) Duration of decision stage (in days)	(3) Hazard ratio - finishing decision stage	(4) Hazard ratio - finishing decision stage	(5) Likelihood of decision completion in 1 month	(6) Likelihood of decision completion in 1 month
Entered after decree application	3.426	5.217	0.079	0.164*	-0.039	-0.009
	(6.778)	(9.298)	(0.072)	(0.088)	(0.040)	(0.055)
Trend	0.706***	0.798***	-0.014***	-0.016***	-0.007***	-0.008***
	(0.176)	(0.201)	(0.002)	(0.002)	(0.001)	(0.001)
Interaction	-2.203***	-2.609***	0.003	0.001	0.006**	0.006
	(0.422)	(0.599)	(0.005)	(0.006)	(0.003)	(0.003)
Constant	104.072***	101.700***			0.440***	0.474***
	(8.565)	(7.696)			(0.077)	(0.071)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes	No	Yes
Pre-mean	63.972	63.972			0.494	0.494
Pre-sd	83.380	83.380			0.500	0.500
R-Squared	0.061	0.064			0.155	0.154
Observations	3851	3496	4109	3738	4116	3745

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 6: **Impact on the pre-trial stage: Channels**

	(1) Number of pretrial hearings	(2) Number of pretrial hearings	(3) No pre-trial	(4) No pre-trial	(5) Pre-trial likelihood of being heard	(6) Pre-trial likelihood of being heard	(7) Judge more strict	(8) Judge more strict
Entered after decree	-1.423***	-1.913***	0.183***	0.238***	0.051***	0.062***	0.055***	0.062***
application	(0.419)	(0.520)	(0.030)	(0.038)	(0.012)	(0.018)	(0.015)	(0.023)
Trend	-0.059***	-0.043***	0.003***	0.002**	-0.000	-0.001	-0.001**	-0.001**
	(0.013)	(0.015)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
Interaction	0.078***	0.065*	-0.008***	-0.008***	0.001	0.000	0.005***	0.005***
	(0.028)	(0.035)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)
Constant	7.322***	8.458***	0.137***	0.092***	0.859***	0.945***	0.159***	0.139***
	(0.411)	(0.618)	(0.033)	(0.028)	(0.014)	(0.016)	(0.016)	(0.018)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes	No	Yes	No	Yes
Pre-mean	8.057	8.057	0.120	0.120	0.887	0.887	0.154	0.154
Pre-sd	6.417	6.417	0.325	0.325	0.131	0.131	0.182	0.182
R-Squared	0.135	0.142	0.096	0.104	0.206	0.217	0.067	0.069
Observations	5169	4667	5169	4667	5137	4638	3434	3141

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 7: **Impact on the decision stage: Channels**

	(1) Number of decision stage hearings	(2) Number of decision stage hearings	(3) Decision stage likelihood of being heard	(4) Decision stage likelihood of being heard
Entered after decree application	-0.087	0.012	0.022	0.046
	(0.296)	(0.379)	(0.026)	(0.039)
Trend	0.020***	0.022***	-0.005***	-0.006***
	(0.007)	(0.008)	(0.001)	(0.001)
Interaction	-0.079***	-0.089***	0.006***	0.007**
	(0.019)	(0.025)	(0.002)	(0.003)
Constant	2.579***	2.414***	0.825***	0.748***
	(0.357)	(0.340)	(0.022)	(0.035)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes
Pre-mean	2.615	2.615	0.773	0.773
Pre-sd	3.442	3.442	0.247	0.247
R-Squared	0.029	0.030	0.307	0.298
Observations	4377	4000	4137	3768

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 8: **Impact on quality**

	(1) Pre-trial insufficient	(2) Pre-trial insufficient	(3) Decision postponed	(4) Decision postponed
Entered after decree	0.026	0.017	-0.009	-0.004
application	(0.029)	(0.042)	(0.022)	(0.029)
Trend	0.001**	0.001	0.001***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Interaction	0.001	0.003	-0.001	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)
Constant	0.130***	0.145***	0.017	0.056**
	(0.034)	(0.044)	(0.018)	(0.022)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes
Pre-mean	0.120	0.120	0.054	0.054
Pre-sd	0.325	0.325	0.227	0.227
R-Squared	0.018	0.020	0.043	0.045
Observations	4137	3768	4137	3768

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 9: **Differential impacts by case difficulty: claim amount**

	(1) Duration of pre-trial hearings (in days)	(2) Likelihood of pre-trial completion in 4 months	(3) Number of pretrial hearings	(4) Pre-trial likelihood of being heard	(5) Judge more strict	(6) Pre-trial insufficient	(7) Decision postponed
Above median claim	30.252***	-0.118***	1.704***	-0.005	-0.006	0.062*	0.089***
amount	(10.089)	(0.039)	(0.473)	(0.012)	(0.023)	(0.034)	(0.023)
Entered after decree	-26.538**	0.107**	-1.136*	0.039***	0.042	-0.034	-0.007
application	(13.480)	(0.052)	(0.633)	(0.014)	(0.029)	(0.040)	(0.027)
Abm claim amount X	-1.824	-0.011	-0.187	-0.010	0.061*	0.096*	-0.047
Entered after	(15.904)	(0.060)	(0.732)	(0.015)	(0.033)	(0.054)	(0.040)
Trend	-0.063	-0.001	-0.010	-0.001**	-0.001	0.000	0.001*
	(0.425)	(0.002)	(0.020)	(0.000)	(0.001)	(0.001)	(0.001)
Entered after decree	-1.065	0.001	-0.006	0.002**	0.007***	0.009***	0.002
application X Trend	(0.845)	(0.003)	(0.039)	(0.001)	(0.002)	(0.003)	(0.002)
Abm claim amount X	-1.525***	0.003	-0.049**	0.001	-0.000	0.002	0.002***
Trend	(0.524)	(0.002)	(0.024)	(0.000)	(0.001)	(0.002)	(0.001)
Triple interaction	0.426	-0.003	0.023	-0.001	-0.003	-0.012***	-0.006**
	(1.134)	(0.004)	(0.048)	(0.001)	(0.002)	(0.004)	(0.003)
Constant	170.234***	0.687***	6.032***	0.948***	0.148***	0.135***	0.050
	(14.062)	(0.055)	(0.710)	(0.017)	(0.024)	(0.048)	(0.031)
P-value: effect for large cases	.0285	.0488	.02	.0437	0	.2072	.1282
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adjustment period	No	No	No	No	No	No	No
Pre-mean	100.071	0.669	5.878	0.927	0.149	0.093	0.024
Pre-sd	117.008	0.471	5.338	0.117	0.189	0.291	0.153
R-Squared	0.224	0.153	0.177	0.221	0.090	0.031	0.053
Observations	3380	3532	3532	3522	2208	2851	2851

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 10: **Differential impacts by case difficulty: claim amount (without adjustment period)**

	(1) Duration of pre-trial hearings (in days)	(2) Likelihood of pre-trial completion in 4 months	(3) Number of pretrial hearings	(4) Pre-trial likelihood of being heard	(5) Judge more strict	(6) Pre-trial insufficient	(7) Decision postponed
Above median claim	27.530**	-0.089*	1.477**	-0.008	-0.030	0.097**	0.103***
amount	(13.121)	(0.052)	(0.612)	(0.013)	(0.026)	(0.040)	(0.028)
Entered after decree	-55.798***	0.229***	-2.678***	0.049***	-0.002	-0.021	0.005
application	(17.159)	(0.065)	(0.794)	(0.019)	(0.043)	(0.054)	(0.037)
Abm claim amount X	26.678	-0.116	1.405	-0.011	0.123***	0.073	-0.046
Entered after	(19.781)	(0.076)	(0.916)	(0.018)	(0.045)	(0.070)	(0.057)
Trend	0.304	-0.002	0.011	-0.001*	-0.001	-0.000	0.001
	(0.515)	(0.002)	(0.024)	(0.001)	(0.001)	(0.001)	(0.001)
Entered after decree	-0.082	-0.004	0.042	0.001	0.009***	0.010**	0.002
application X Trend	(0.961)	(0.004)	(0.045)	(0.001)	(0.003)	(0.004)	(0.003)
Abm claim amount X	-1.596**	0.004*	-0.057**	0.001	-0.001	0.003*	0.003***
Trend	(0.618)	(0.002)	(0.028)	(0.001)	(0.001)	(0.002)	(0.001)
Triple interaction	-1.397	0.002	-0.068	-0.000	-0.005*	-0.014**	-0.008*
	(1.330)	(0.005)	(0.056)	(0.001)	(0.003)	(0.005)	(0.004)
Constant	84.140***	0.627***	5.080***	0.788***	0.156***	0.109*	0.056**
	(14.914)	(0.063)	(0.791)	(0.022)	(0.040)	(0.062)	(0.028)
P-value: effect for large cases	.0735	.0771	.0797	.0439	.0001	.42	.3738
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adjustment period	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	100.071	0.669	5.878	0.927	0.149	0.093	0.024
Pre-sd	117.008	0.471	5.338	0.117	0.189	0.291	0.153
R-Squared	0.233	0.151	0.183	0.225	0.096	0.032	0.057
Observations	3051	3198	3198	3188	2019	2596	2596

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.